British Journal of Anæsthesia

VOL. IX. NO. 4. JULY, 1932.

EDITORIAL.

IN this last number of the journal for the current medical year we may be forgiven for a moment's retrospection and introspection. We can look back with some satisfaction, for we have been the happy recipients of a good many complimentary messages, which encourage us for the future and yet do not allow us much complacency, because we realize that where praise is earned it has been by the efforts of our contibutors rather than of ourselves. With regard to this matter we would hold out a beckoning hand to all our young readers. We would like to see the stalwarts who support and who fill our pages so nobly more often relieved of their duties by the aspiring pens of the younger generation. If they do not supply us with articles, descriptive of their work and the new lines along which they are developing it, at any rate let them work off in our correspondence columns the ardent feelings which they doubtless hold with regard to the socio-economic aspects of their special branch of medicine. Mention of this leads to mention of the newly forming "Association of Anæsthetists" which we notice on another page. This body will, we hope, do much to further that development of our specialty which is so well adumbrated in the article we publish from Dr. Ross Mackenzie. Particularly, we imagine, will the item of a "Diploma in Anæsthetics" appeal to our readers. There is undoubted need for some means by which the discrimination can easily be made between the sheep and the goats, i.e. between those who are anæsthesists by name only and those who are the real thing by position, experience and

competence. It looks as though the new body will attract the support of leading anæsthetists all over the country. When it has done so and has added to its ranks most of those worthy of the name of anæsthetist it should be in a position to work steadily and effectively for those reforms the need of which is, we know, acutely felt by all who have at heart the interest and welfare of their calling. We would close these few remarks by a special word of acknowledgment to the officers of the Anæsthetic Section of the British Medical Association, especially to the President, Sir Francis Shipway, for the kindness with which they have given us the opportunity of publishing Professor Finsterer's comprehensive paper. Too extensive for its own journal to publish in full, the Association has been good enough to put it in our hands even before its reading by the Professor. This is what we call a piece of sporting journalism that deserves and gets our warmest thanks.

OBITUARY.

Hugh R. Phillips, M.D.

With much regret we announce the death of Dr. Hugh Phillips, whose name has often figured in our columns. Dr. Phillips died in a nursing home in London on June 21st, shortly after an operation. He was Senior Anæsthetist to the Italian Hospital, and had been for many years Anæsthetist to the Great Ormond Street Hospital for Children, and to the Hospital of SS. John and Elizabeth. Hugh Phillips was the son of a successful London practitioner. He served in the Boer War and in the Great War as a Naval surgeon, and later held a commission in the R.A.M.C. Besides his extensive private practice in anæsthetics and hospital work, Freemasonry took up much of Hugh Phillips' time. He was a past Grand Deacon of England. Phillips was a Cavalier of the Order of the Crown of Italy, and a J.P. In his younger days at Edinburgh University Hugh Phillips was famous as a heavy-weight boxer; being champion of his University. He leaves a widow and son, to whom we tender our sincere sympathy.

THE CHOICE OF AN ANÆSTHETIC FOR ABDOMINAL SURGERY.*

By Professor H. FINSTERER, M.D., Vienna.

THE invitation to read an opening paper on the choice of an anæsthetic for abdominal surgery at the Centenary of the British Medical Association is the greatest honour ever conferred upon me. First of all I beg to offer my thanks for the invitation, which I esteem as a high distinction, to the President of the British Medical Association, Lord Dawson of Penn, to the President of the Section of Anæsthetics, Sir Francis Shipway, and to all the other officers. On account of my inadequate knowledge of the English language I am not able to carry out my task as well as I should like to, and so I must ask for your kind consideration.

In order to get acquainted with the present standpoint of anæsthesia for abdominal operations I have asked 200 members of the German Society of Surgery, who are directing clinics of Universities or larger hospitals, to tell me their views and experiences. The 120 answers which I have received have been a great help to me, and I beg to thank all these gentlemen for their trouble. My own experiences refer to 5,172 abdominal operations performed by me within 25 years. They were performed under various methods of anæsthesia, and have been worked out statistically. Regarding the short time for, and the limited extent, of this opening paper I can only refer to the most important publications that have appeared since the paper which I had the honour to read six years ago to the British Medical Association at Nottingham.

In abdominal operations anæsthesia is of much higher importance than in any other operation; for in all acute diseases of the abdomen, as well as in chronic disease of the gall bladder and the stomach, the parenchymatous organs, most of all the liver, are already injured, so that further

* Read in opening a discussion in the Section of Anæsthetics at the Centenary Meeting of the British Medical Association, London, 1932.

damage caused by general anæsthesia might be followed by death from insufficiency of those organs. As the protective ferments have been diminished the danger of post-operative peritonitis and of consequent death is increased. Generally, anæsthesia is regarded as dangerous only if during the operation the patient dies. The frequency of deaths from narcosis depends on whether all the fatalities during narcosis, even if they are caused by the gravity of the disease itself, as well as all the cases where death occurred several hours after the operation when the patient did not awake from the narcosis, are considered as being due to narcosis or not. By strictly regarding the contra-indications (severe heart or pulmonary diseases and other conditions), by improving the technique of narcosis, the number of deaths immediately from narcosis has greatly decreased, so that they are scarcely to be feared. Of greater importance are the fatalities from late injury, which are connected more with the duration and depth of the narcosis than with its technique.

Chloroform caused most of the immediate deaths, even in small operations, especially at the beginning of the narcosis. It injures the parenchymatous organs, particularly the liver (Widal-Abrami and Hutinel), and causes death from insufficiency of the liver (Stierlin, Sprengel, Schnitzler, jun., Reichel, etc.); it regularly induces gastro-intestinal atony with frequent vomiting, often leading to death (A. Payer). Therefore, chloroform has nearly been given up. It is used by only 15 out of 120 German surgeons, in the form of Billroth's mixture, either to begin the narcosis or to deepen an insufficient ether narcosis, for instance in drunkards. Also in children its use is not without danger and should be avoided (Schmieden and Sebening). I have not used chloroform for 20 years, because even with drunkards I could attain the necessary depth of narcosis by combining ether narcosis with local anæsthesia of the abdominal wall and mesenteries.

Ether is the most commonly used anæsthetic, not only in Germany but also in U.S.A. (according to Stanton by 85 per cent of all surgeons). It is used either as a drop narcosis with an open mask or mixed with oxygen in a special apparatus (Roth-Dräger) or in form of re-breathing with the mask of Ombrédanne. To diminish the quantity

of ether, and to avoid the mucous secretion, the patients receive morphine or pantopon together with atropine or scopolamine. With ether there are much less immediate risks than with chloroform; deaths from ether narcosis are infrequent. In the material of 16 Viennese surgeons Breitner has found 23 deaths from narcosis in 60,000 ether narcoses (1 : 2,610). At the clinic of Bier a death did not occur in 16,843 cases of ether narcosis (Martin). The frequency of fatalities differed in the answers to my inquiry. Most of the surgeons did not have any deaths at all (e.g. Denk's clinic with 5,460 cases of ether narcosis). Fourteen surgeons have to report 24 deaths from narcosis, among which there are three when the mask of Ombrédanne was used.

In my own practice I have had 610 cases of narcosis, i.e. combined anæsthesia (local anæsthesia and more than 100 cubic centimetres of ether used with open mask) with three deaths. Death is mostly caused by paralysis of the respiratory centre. We have not seen clonic convulsions, as reported by Wilson, Pinson, and Sykes, in young patients.

Of much greater importance are the so-called "deaths from operative shock" occurring three to five days after the operation, which are nearly always protracted deaths from narcosis (as I asserted 20 years ago), for they do not occur when deep narcosis is avoided. These fatalities are mostly called death from heart insufficiency, but according to Eppinger they are not caused by primary weakness of the heart but by a primary weakness of circulation with a bad distribution of blood, which in its turn is the consequence of injury from the narcosis and the operative trauma (resorption of products from disintegration of cells, similar to histamine). According to my own experiences the narcosis is of the highest importance; the operation itself, when delicately done, is of minor influence, for the weakness of circulation is absent when one operates under local anæsthesia. It must, therefore, be the narcosis which diminishes the resistance of an already damaged organ and finally causes paralysis of the vasomotor centre and death.

Some of the deaths are caused by injury of parenchymatous organs, especially the liver, the kidney and the suprarenal gland. By the experimental researches of Bourne,

Crile, Müller, Saison, and others it was found that similar injuries occur not only with chloroform but also with prolonged ether narcosis. The same changes in the liver as are caused by the use of chloroform were shown in autopsy by Latzko after each prolonged ether narcosis, even when the mask of Ombrédanne was used. The liver is the most important regulative centre in metabolism (Rehn). It may have been injured in advance by different diseases of the abdomen (acute inflammations, chronic affection of the gall bladder, malignant tumours, etc.), so that, when the damage by the ether narcosis is added, there results an insufficiency of the liver (chemical decompensation) whereby the reserve of alkalies is diminished and acidosis increases. After every ether narcosis hepatic injuries may be shown by the clinical test of the function of the liver (Bergmann and von Eilbott). If the liver has already been damaged in advance its post-operative injuries become more distinct and last longer. (Boshamer). Deaths from injury to the liver are proved by autopsy (Balkenhausen, von Lacqua, in five cases of the clinic of Küttner). In the material collected by me 18 deaths from liver injury are reported by six surgeons; one of them (Bayer) describes six fatal cases in 364 operations on the gall bladder done under ether narcosis.

In my own material of 610 operations with narcosis there were 13 deaths (2.2 per cent), during the first five days, with symptoms of weakness of the circulation. Ether, alone, had been used in eight cases; in two cases some chloroform had been added; nitrous oxide had been applied in three cases. Of 122 resections of the stomach, for ulcer, performed under general anæsthesia, seven patients (5.7 per cent) died from weakness of circulation.

Deaths from atony of the stomach after ether narcosis are rarer than after chloroform narcosis. Once they were erroneously called a "vicious circle," especially after operations on the stomach. They were also named arterio-mesenteric obstruction of the duodenum, though relaparotomy or autopsy always showed the same typical findings—stomach, duodenum and upper jejunum dilated to the utmost, nearly to the size of an upper arm, filled with liquid contents, but no peristalsis. The dilated intestine gradually narrows down to the normal

size. There is no peritonitis; no adhesions. Without re-laparatomy or autopsy these cases cannot be diagnosed with certainty; they are mistaken for a high mechanical obstruction of the small intestine. The cause is probably a central paralysis of a common innervation centre for stomach and upper jejunum, caused by ether. This paralysis may appear spontaneously and may be caused by toxines after infectious disease (encephalitis). It is incurable and ends in death in spite of all measures (two cases observed by myself). According to my inquiry 13 surgeons have reported 34 deaths from atony of the stomach and intestines after ether narcosis. I myself observed five deaths from gastro-intestinal atonies in 610 operations under ether narcosis, three of which were ascertained by re-laparotomy.

A man, aged 42 years, with healthy organs, insisted on general narcosis in an operation for chronic appendicitis and cholecystitis. This was performed under ether narcosis with the drop method. The amount used was 290 cubic centimetres of ether; this gave good and regular narcosis. From the second day increasing vomiting occurred in spite of repeated lavage of the stomach (three or four times a day), physostigmin, pituitrin, neohormonal injection, etc. The pulse was 80, the stool and gases were normal. An the fifth day acceleration of the pulse to 130 occurred, with fœcal vomiting. Re-laparotomy with local anæsthesia was, therefore, performed, under the diagnosis of high intestinal obstruction from adhesion of an intestinal loop to the bed of the gall bladder. The stomach was enormously overdilated, highest loop of jejunum as large as an upper arm, filled with liquid, and the circumference gradually narrowing after one and a half metres. The remainder of the intestine and peristalsis were normal. There were no adhesions and no peritonitis. There was some fluid in the abdomen (bacteriological finding: sterile). Puncture of the paralysed loops, enterostomy, without any success. Death occurred after 24 hours. The same state presented itself in a patient, aged 38 years, after total colectomy for megacolon in combined anæsthesia (local anæsthesia and 200 cubic centimetres of ether) and in a man, 52 years of age, after radical operation for gastro-jejunal ulcer (local anæsthesia and 300 cubic centimetres of ether.)

As I have not had a death from atony after 4,562 operations with local anæsthesia, I find this a more appealing argument than the evidence, afforded by experiments on animals, that the fatal atony is not connected with the operation itself but only with the narcosis, and that it must

be caused by a central paralysis of a special centre of innervation.

With prolonged ether narcosis the normal powers of resistance, especially the phagocytosis, are seriously injured or quite destroyed; this is a fact ascertained by the experimental researches of Klein and Coxwells, Müller, etc. Therefore the smallest infection, insignificant in operations under local anæsthesia, leads here to death from peritonitis. In my own material deaths from peritonitis after operations under narcosis are more often to be found than after local anæsthesia (for instance in the gastric resection for ulcer, 4.4 per cent to 1.4 per cent). It is just in simple and quickly performed operations that they occur, but they are not observed in extremely difficult and longer lasting radical operations for gastro-jejunal ulcer.

Ether narcosis favours also pneumonia, because the normal power of resistance in the lungs is destroyed by the ether narcosis (shown by experimental researches by Snel, Chapman, Müller and others) and bacteria in the bronchi or the aspirated material may easily penetrate into the tissue of the lungs. There are also other important causes of pneumonia—refrigeration by the ether (prevented by Tiegel by the use of a special apparatus which first warms the ether vapour), retention of secretions through incomplete expectoration, or, more rarely, embolism (Henke). Fatalities from pneumonia depend directly upon the action of the heart, which is damaged by ether as well as by chloroform (E. König). Mortality from pneumonia after ether narcosis has diminished by improved technique and by a very exact after-treatment, from 9.2 per cent (von Lichtenberg) to 3.7 per cent (clinic Ranzi-Starlinger) and 1.31 per cent in my own material.

Thrombosis and death from lung embolism may be partially dependent on the use of ether narcosis. It is remarkable that at the time ether narcosis became the method of choice instead of chloroform narcosis the frequency of death from embolism rose from 1.4 per cent to 4.4 per cent (Nordmann). On the other hand, at Sudeck's clinic the frequency was diminished to one-tenth of the previous number when in all more difficult operations ether narcosis was replaced by nitrous oxide narcosis. Rabinovich (Leeds)

found in 80 per cent of the clinically examined patients an increase in the coagulation of the blood after ether narcosis, even in cases in which an operation was not performed. In his experimental work Mulzer observed an increase of coagulation in the blood-vessels, caused by damage to the erythrocytes through the narcotic. Certainly, in consequence of the resorption of waste products from the operative field, globuline and fibrinogen in the blood are augmented and the tendency to agglutination rises (Seemen). The increasing frequency of intravenous injections is also of some importance (Schleussing). In my own material the frequency of death from embolism is relatively small (0.3 per cent) compared with three per cent in Nordmann's material; it is higher with operations under ether narcosis (0.98 per cent) than under local anæsthesia (0.24 per cent), being the highest in gastric resection for cancer (1.1 per cent). It is remarkable that death from emboli is absent in cases of severe hæmorrhage from gastric or duodenal ulcers and in cases of long-lasting jaundice through duct obstruction.

To avoid direct injury of the lungs Burkhardt recommended the intravenous ether narcosis. This method, however, did not hold good because of its drawbacks (thrombosis). Borchardt used intravenous isopral-ether narcosis with good results. Rectal ether narcosis in form of a 33 per cent mixture of ether and oil, after Gwathmey, is not applied in Germany though the psychic effect is good.

Ethyl chloride is often used in the beginning of ether narcosis; most surgeons believe it to be absolutely without danger, but deaths are reported in the literature by Hofmann, Jäger, Kausch, Renner and others. By inquiring at 150 German clinics and hospitals, including departments for nose and throat diseases, Hayward gathered 24 deaths which happened in Germany in recent years (until 1928). Most of these deaths (14) occurred in short narcosis ("Rausch"). To my inquiry von Verebely answered that he observed three deaths from ethyl chloride ("Rausch"). Some surgeons use it for longer lasting narcosis. Ingebrigtsen had 300 narcoses without a death. Caillaud uses a special apparatus (Collin) to secure an exact dosage and a good mixture with air. With this apparatus he administered over 3,000 longer lasting

narcoses without an accident. I do not like ethyl-chloride at all, as I saw a death at Hacker's clinic 24 years ago

Narcosis with nitrous oxide is somewhat difficult in its application, needing a special apparatus and an expert anæsthetist. It is a little more expensive, but does not injure the parenchymatous organs. It is, therefore, held to be the least dangerous general natcotic. Deaths during an operation are caused by technical mistakes or defects of the apparatus (deaths described by Hahn). They can easily be avoided. Besides the American statistics, the clinic Sudeck reports on several thousands of cases of narcosis with nitrous oxide without a death (Schmidt), as well as the clinic Denk (Domanig). In protracted narcosis, injuries of circulation seem to be possible, at least I think that in three cases narcosis with laughing-gas was the cause of death on the third to fifth day from insufficiency of the circulation.

Narcylen is seldom applied on account of its explosiveness (used by only five surgeons, two of whom have already given it up).

Avertin (tribromethylalcohol) has been a good deal applied these last five years, as is to be seen from the detailed paper written by Anschütz, Specht and Tiemann and from the answers to my inquiry. Forty-five surgeons out of 120 are using avertin, most of them only in selected cases, but 10 surgeons employ it as the method of choice. Its greatest advantage over general narcosis and local anæsthesia is that the mind of the patient is absolutely excluded. On account of this quality alone it could be the method of choice for all surgeons if the patient's life were not endangered by it. Specht calculated the frequency of unavoidable deaths from avertin as 1 : 10,000; that is the lowest rate of mortality in all methods of anæsthesia. In this calculation all deaths from technical failures are excluded as well as the deaths from gastro-intestinal atony, even when, by re-laparotomy or autopsy, the absence of peritonitis was stated and deaths occurring were ascribed to the use of avertin by the authors themselves. As I mentioned before, atony is caused by a central paralysis due to the narcotic (that is, by avertin), whilst it is absent absolutely in local anæsthesia. If in narcotizing with avertin one does not insist on total narcosis,

and if the dosage for the basal hypnosis is calculated for each single case following the scheme of Domanig (clinic Denk) completed by Anschütz, the number of deaths can be restrained but, unfortunately, not quite avoided, as is to be seen from the cases reported by Burk, Domanig and Schönbauer. These unexpected deaths cannot even then be avoided when patients to whom narcosis is a dangerous risk—for instance, persons who suffer from diseases of the liver, of the kidney, or from extreme cachexia—are excluded from narcosis with avertin, as is desired by many of its followers. This insecurity was the reason why I did not use avertin for basal hypnosis with my patients, but restricted myself to preparing the patient with an appeasing injection of pantopon and morphine, as the first duty in choosing an anæsthetic is "*primum non nocere*. Moreover, in many cases of basal hypnosis the amount of ether required is greater than when local anæsthesia of the abdominal walls and mesenteric anæsthesia are combined with ether narcosis. As to the details of the narcosis with avertin, as well as of Kirschner's intravenous "Rausch" narcosis, I must refer to the excellent and complete paper written by Anschütz, Specht and Tiemann.

Pernocton, a derivative of barbituric acid, was recommended as another efficacious and harmless drug for basal hypnosis by Bumm (clinic Hildebrand) in the dosage of one cubic centimetre (0.1) per 12½ kilogramme body-weight; but as in his material of 66 operations there are quoted eight deaths, five of which happened shortly after the operation from so-called operative shock, which probably was caused by intoxication with pernocton, this narcotic cannot be called a harmless one. In fact, fatalities occurred when the narcotic was tried in few cases; for instance Haberer reported one death after using the narcosis in eight cases, Walzel (Eichelter) after three cases, Linsmeier (Rippel) had two fatalities in 82 cases. On the other hand, Husemann did not see any damage in 800 cases, Bode in 1,300 cases, Snethlage in over 1,800 cases of basal hypnosis. Answering to my inquiry 17 surgeons have replied that they had tried pernocton but had to complain of five deaths from it. The method was abandoned by nine surgeons; eight are still using this narcotic, especially in cases in which avertin cannot be used because

of an injured liver; three surgeons use pernocton in almost all cases. With excitations the ordinary amount of ether, or even more, is required, and the combination with local anæsthesia is not advisable, though Colmers reports good experiences in over 100 cases.

Local Anæsthesia.

In all methods of local anæsthesia consciousness is intact, so that it is advisable to reduce the sensibility of the patient to injections by a previous injection of pantopon or morphine. This procedure also appeases highly nervous patients. As a rule my patients get 0.02 gr. pantopon one hour before the operation and 0.01 to 0.02 gr. morphine combined with 0.003 to 0.0005 atropine a quarter of an hour before operation. While Schleich's method of infiltrating the incision line is a real local anæsthesia, we now perform a regional or conductive anæsthesia by which the sensitive nerves are blocked centrally from the operative field within the abdominal wall or in the mesenteries, or in the spinal column, or, finally, within the dural sac. Only Wischnevsky is still using Schleich's method; he even applies, intra-abdominally, large quantities of the fluid which inundates the field of operation. The nearer the narcotic is brought to the spinal cord the greater are the risks of local anæsthesia; the greatest danger is in spinal anæsthesia (Braun).

In every laparotomy an exactly performed anæsthesia of the abdominal wall is most important. It has, also, to be executed when the operation is performed with paravertebral anæsthesia owing to the abundant anastomosis. It also may be added to spinal anæsthesia, for when the efficiency of the spinal anæsthesia has ceased the abdominal walls will still be insensible (for at least two hours), so that an additional narcosis for the closing of the abdominal wall can be avoided. Anæsthesia of the abdominal walls is sufficient for small abdominal operations (gastrostomy, jejunostomy, colostomy) and for gastro-enterostomy too. In all other operations (in all resections) at least those nerves that are in the neighbourhood of the mesenteric vessels have to be blocked by an injection of novocain at the base of the mesenteries. This mesenterial anæsthesia, recommended by me 20 years ago, and in principle

similar to the procedure called "anæsthesia of the vessels" by Drüner and Härtel, shows painless ligating of the mesenteries, therefore making possible every resection of the small intestine and of a movable tumour of the stomach. It is absolutely without danger, prevents all shock (Crile) by interrupting the conduction of pain to the brain, and should, therefore, be applied in all operations to avoid shock and to spare the general narcosis, even if a general narcotic has been administered. When the organ is fixed, the peritoneum of the posterior abdominal wall has also to be anæsthetized by paravertebral or by splanchnic anæsthesia, or there must be a short ether "Rausch," or a short narcosis with laughing-gas for the time needed in mobilizing the organ.

Posterior splanchnic anæsthesia (Kappis) has the advantage that it may be applied in every case, even if there are adhesions in the upper abdomen, and that the introduction of the needle after the abdomen is opened can be avoided. It is said to be very dangerous because in the early days of its use there occurred several deaths (8) from intravenous or intraspinal injection. As I pointed out in my book on "Local anæsthesia methods and its success" these fatalities can be avoided if the injection is applied to the patient whilst in a sitting position, and not whilst lying down. Hilarowicz uses the modification of Roussiel in which the needle is introduced above the twelfth rib and the splanchnics are interrupted within the thorax above the diaphragm. In using this modification the intravenous, as well as the lateral, spinal injection can be avoided. The method of Kappis, however, is not so dangerous as is said (Schroeder); there being only one death in 343 cases. Kappis himself did not have a death in over 1,500 cases. Hustinx, also, reports on 663 cases without a death. The method has been used by seven German surgeons during recent years. In more than 400 cases a death did not occur.

Hilarowicz (Lwow), in answer to my special inquiry, states that he performed 785 posterior splanchnic blocks without any accident. Of late he has been using percaine with long duration of anæsthesia (four to six hours), It is the method of choice for all operations on the stomach and gall bladder. In an extensive paper on splanchnic anæsthesia Takats

gathered from the available literature 2,475 cases with eight deaths.

The death reported by Kutscha-Lissberg must be excluded because a splanchnic block was combined with high spinal anæsthesia in a case of diffuse peritonitis following a rupture of the ascending colon, through trauma, three days previously. This death is due more to the advanced peritonitis than to the high spinal anæsthesia; it cannot be counted with fatalities of splanchnic anæsthesia. The same holds good with the deaths reported by Jirasek, as in these cases high spinal anæsthesia was added to the splanchnic block.

Graef reported the history of a man, 61 years old, with an acute colon obstruction caused by a cancer of the splenic flexure. In spite of the fact that the patient was brought to the operating room in a state of collapse, he was given splanchnic block for a simple caecostomy. I believe it was a great mistake to use splanchnic block for this operation, which is better performed under paravertebral anæsthesia, if local anæsthesia of the abdomen combined with a deep injection in the fossa iliaca, as for appendectomy, would not suffice. In Denk's case an advanced diffuse peritonitis caused by a perforation of a strangulated hernia was present. As the patient showed signs of collapse before the operation, Denk believes that the fatality probably had no connexion with the injection.

In four cases the death was caused by the splanchnic block. In the observations of Heller and of Klautz an intravenous or intraspinal injection happened, with immediate death. Borszeky punctured the suprarenal gland, as shown by the post-mortem. In Graf's case the dose was too high (100 cubic centimetres of a half per cent solution of novocain was used for the splanchnic block for a cachectic patient.) I never use more than 70 cubic centimetres. If one judges the mortality of posterior splanchnic anæsthesia in the same manner as Specht did that of avertin narcosis, excluding the deaths from technical faults, then the mortality of posterior splanchnic anæsthesia is not 1:343, but 1:5,895, as I could find in the literature and in the answers to my inquiry at least 5,895 cases of splanchnic blocks.

In the early years of splanchnic anæsthesia I myself have

used the method of Kappis 72 times without complication. I am now using anterior splanchnic anæsthesia, after Braun, if I plan to do a radical operation after exploratory laparotomy. Whereas Braun and Haberer use 100 cubic centimetres of a half per cent solution of novocain, I am injecting only 70 cubic centimetres, as I have seen slight symptoms of intoxication (tachycardy, paleness, air-hunger) after having used larger doses. In cachectic or gravely anæmic patients I use a quarter per cent solution of novocain instead of a half per cent solution, and get satisfactory anæsthesia with only 50 cubic centimetres. With stomach resections I needed an auxiliary general narcosis 115 times in 1,324 cases (8.6 per cent) of splanchnic anæsthesia. Lately I have been using laughing-gas narcosis for this purpose. Among these 115 cases general narcosis was given in 24 cases only during the time of introducing the splanchnic needle in order to avoid pain. Fifteen times the operation was started under narcosis on the special demand of the patient; but after the splanchnic anæsthesia had been applied narcosis was totally left off and the patients were awake and absolutely quiet during the whole operation. In 45 cases, mostly radical operations for gastro-jejunal ulcer, narcosis had to be given, either in the beginning to separate the adhesions, or when the effect of the splanchnic anæsthetic had gone before the operation was finished. It cannot, therefore, be said that the anæsthesia failed in these cases. Only in 31 cases (2.3 per cent) did the patients feel pain, in spite of the splanchnic anæsthesia, so that I had to resort to general narcosis.

In callous tissue it may happen that the diffusion of the novocain is not sufficient so that the anæsthesia is incomplete. Pannett, therefore, has usefully modified the injection by injecting 30 cubic centimetres of a one per cent solution of novocain at the typical place between the aorta and the cava, and by blocking separately the left splanchnic by an injection of 10 cubic centimetres on the left side of the aorta.

If handled with precaution (avoiding intravenous injection) and with an exact dosage, splanchnic anæsthesia occasions few immediate risks. In the opening paper I read six years ago to the British Medical Association I was able to collect 2,373 cases of splanchnic anæsthesia, after Braun's method, without any fatality. As to my inquiry from surgeons, the

number of operations performed under splanchnic anæsthesia (including 1,838 cases in my own material) must to-day reach 9,992. Considering the fact that several surgeons have not given exact numbers, but only reported that splanchnic anæsthesia is used by them in five to ten per cent of all operations on the stomach and on the gall bladder, or that they use splanchnic anæsthesia very often and have not yet observed any death, etc., the total number of cases must be much higher still. Braun's method was used most frequently; by Adam (1,814 cases), Braun-Kulenkampff (951), Payr (over 600), Doberer (438), and Henschen (over 400).

In 242 cases of splanchnic anæsthesia von Verebely has seen one death, half an hour after operation. Autopsy showed embolism of lung caused by thrombosis of the vena iliaca and the vena cava; the vena cava was not hurt, therefore there was no relation to splanchnic anæsthesia. I have had one death, a man aged 42, which occurred during the injection, where, in using a new anæsthetic (pantocain) the solution had been erroneously prepared. At the autopsy, in addition to a small cancer of the stomach which already had spread metastasis in the retroperitoneal lymphatic glands, a small lateral hole was to be seen in the vena azygos, passing atypically in front of the twelfth lumber vertebra. This hole must have been made on account of the somewhat difficult introduction of the needle. As a repeated aspiration had never shown any blood in the syringe an intravenous injection is to be excluded; overdosage by a tenfold dose was the fatal cause. The death reported by Maiditsch (clinic Haberer) occurred in a woman aged 61, with a bad heart (pulse 112 before operation; extrasystoles), in a difficult cholecystectomy, due to simultaneous arrest of the heart's action and respiratory standstill. As the quantity and concentration of anæsthetic used is not communicated, a critical examination of this case is not possible. An intravenous injection is not probable, as the death occurred only some time after the injection, and tonic-clonic cramps were absent. It is possible to assume that in the bad general condition of the patient the ordinary dose (100 cubic centimetres of a half per cent solution of novocain) was relatively too strong. Anyhow, this is the only death in splanchnic anæsthesia (Braun) from resorption of the anæsthetic.

On the whole, splanchnic anæsthesia, especially when used after the method of Braun, is not so dangerous as was asserted by Meeker (1 : 343) and as has since been generally quoted (von Schroeder). In the material now collected by me there are two deaths in 9,992 cases; one, which was caused by an over-dosage, should properly be excluded if calculating the death-rate in the same manner as Specht does with avertin. Thus there is one death in 9,992 cases of splanchnic anæsthesia. Lundy refuses this anæsthesia as a dangerous and ineffective method. In South America splanchnic anæsthesia is used successfully by Guttierezz, Vaquié, Zeno, etc. As to my own experiences, I can maintain that splanchnic anæsthesia when cautiously used (no intravenous injection; individual dosage) includes less immediate risks than high spinal anæsthesia, and in more than 90 per cent of all cases an absolute anésthesia for operations in the upper abdomen is attained.

The procedure of Seidel and Baruch of anæsthetizing the abdominal cavity by filling it with novocain solution became successful after the introduction of percain by Henschen. Using this method Mandl experienced one death, after forty hours, from over-dosage (150 cubic centimetres to a half per cent solution of percain). Perhaps pantocain, recommended by Pfitzner for diffusing anæsthesia, will be useful and less dangerous. It would be an advantage thus to get a total and also a harmless anæsthesia of the whole abdominal cavity.

Paravertebral anæsthesia, recommended by me 20 years ago for certain abdominal operations in which the injection of one side is sufficient (operations on the gall bladder, resections of the large intestine) is not advisable in stomach operations even if, according to Mostkovyi, only three injections on each side of the spinal column are applied, instead of 12 to 18 injections. For that reason this method has few followers (Négrié and Rolland, Quarella, and four surgeons of my inquiry, whereof Adam had 240 cases). I myself used paravertebral anæsthesia 174 times, mostly in resections of the large intestine (96 cases), to mobilise the fixed colon ascendens or descendens. For the resection itself mesenteric anæsthesia must be added. In these 174 cases the anæsthesia was insufficient and narcosis necessary in 26 cases.

Parasacral anæsthesia, after Braun, is sufficient only in

operations on the rectum and on the perineum, otherwise the plexus hypogastricus has also to be blocked by an injection of 30 cubic centimetres of novocain, applied either from behind on the lateral side of the fifth lumbar vertebra or, when the laparotomy is perfected, on the anterior surface of the fifth lumbar vertebra below the division of the arota. The method was used by Frigyesi in 1,745 gynæcological laparotomies with 96.4 per cent complete anæsthesia; neither complications nor death were observed. In the sacral operation for rectal cancer this combined parasacral anæsthesia is generally sufficient (39 times in 50 cases) even if the cancer is highly located. In the abdominal-sacral resections, especially with fat men, general narcosis is required either at the beginning or, in long lasting operations, for a short time at the end. Among 76 cases only 32 could be operated on without any general narcosis. Parasacral anæsthesia is absolutely harmless if intravenous injection is avoided (it is very important never to make an injection when the needle is at rest). Burgkhardt did not have a death in over 1,000 cases, nor has Frigyesi in 1,745. In my 126 cases of parasacral anæsthesia there was one fatality, a woman of 63; during the sacral resection of a fixed cancer of the rectum there occurred a most severe bleeding from the superior hæmorrhoidal artery high up on the promontory, so that laparotomy in ether narcosis (80 cubic centimetres ether) was immediately necessary. The woman died from hæmorrhage, blood transfusion being impossible.

Epidural (sacral) anæsthesia in ordinary dosage is sufficient for operations in the pelvis. By using a high Trendelenburg position and larger doses of novocain the anæsthesia can be extended as far as the dorsal segments. This method is used particularly in gynæcological laparotomies (Kehrer, Gaus-Ammon), but is not harmless. High epidural anæsthesia, after Doglitti, in which the effective narcotic is injected epidurally, corresponding to the segment of the operative field, is said to avoid the risks of high spinal anæsthesia and to possess only its advantages. The proof for it is still lacking.

Spinal anæsthesia is the most centrally acting method of local anæsthesia and has the great advantages of simple execution, of absolute painlessness and of muscular relaxation, and, therefore ought to become the method of choice if it did

not include a certain danger to life, and if in normal dosage its duration were sufficiently long. Its principal danger lies in the fall of blood-pressure and in the endangering of the respiratory centre, either by direct paralysis from the narcotic or, probably in consequence of the lack of cerebral blood-supply, because of the depression of blood pressure (Rejsek). The higher the anæsthesia the greater its dangers. High spinal anæsthesia is used, therefore, by only a few surgeons (Berard-Mallett and Condamin, Butoiano, Chalier, Dunhill, Hortolomei, Judin, Jurasz, Juvara, Luque, Pannett, Sise, Valdoni). Only 19 out of 120 German surgeons were using high spinal anæsthesia, five of whom discontinued it. Trying to exclude the risks of high spinal anæsthesia in using spinocaine, Pitkin declares this method to be the safest anæsthesia in operations in the upper abdomen. While Adams, Bode, Kelly, and Pannett had success with this method, Donald and Hill found, on trying it, that there exists no real control. Demel (clinic Eiselsberg) gave up the method after two deaths in 250 cases. Kirschner seeks to make a girdle-like spinal anæsthesia the limits of which can be fixed and in which the dose of the narcotic can be administered individually. For that purpose the smallest necessary quantity of the anæsthesic (2. to 2.5 cubic centimetres of a quarter per cent solution of percaine) is injected to that part of the spinal cord which corresponds to the field of operation, thus only blocking these nerve centres. The Trendelenburg position is used and air blown into the dural sac. Kirschner explains the absence of the fall in blood-pressure by the fact that the vasomotor nerves of the inferior extemities are not blocked. Answering my inquiry he wrote that he now uses this anæsthesia only in his abdominal operations, and that he has not had a failure in over 700 cases. Von Verebely, also, has tried this method, but he had a fatal case from respiratory paralysis and knows of two deaths which happened in another hospital at Budapest with the method. Most surgeons are against high spinal anæsthesia because of its risks. Of this opinion, also, are Forgue and Basset in an opening paper at the meeting of French surgeons in 1928. In operations below the umbilicus, spinal anæsthesia is much more used (by 44 out of 120 surgeons),

even in relatively small operations, as for instance appendectomies. The immediate risks are less, but late injuries are likewise possible. In order to prolong the duration of the anæsthesia percaine is used (Flörcken, Latzko, Quarella, Ziegner). In the discussion following Latzko's paper the gynæcologists Adler and Heidler reported six deaths in a relatively small number of gynæcological laparotomies.

In my material only 60 laparotomies were performed under spinal anæsthesia. I gave it up 15 years ago and introduced the combined parasacral anæsthesia, which not only includes less risk but is also efficient for a longer time (two to three hours). If further experience should demonstrate that the efficiency of spinal anæsthesia can be protracted without increasing its risks, which might, perhaps, be attained with pantocain—which is already used successfully by Pfitzner and H. Schmidt—I shall again employ spinal anæsthesia for operations on the lower abdomen.

The most commonly used drug for local anæsthesia is novocain in a $\frac{1}{2}$ to 1 per cent solution combined with adrenalin. So far this has not been displaced by any newer anæsthetic. The maximal dose has been stated to be 0.5, but this is too small, for, on the average, one grain of novocain in a half per cent solution can be used without injury. Of a quarter per cent solution much larger quantities (up to 500 cubic centimetres) can be applied without any symptoms of intoxication. Wischnevsky even injects up to 1,800 cubic centimetres of the quarter per cent solution, the larger part of which flows off after the incision. In cachectic and anæmic patients weaker solutions (a quarter per cent) should always be applied, as these are quite sufficient. In aged patients with extreme arteriosclerosis it is advantageous to add pituitrin instead of adrenalin to the solution of novocain, as recommended by Wermer; I have done so repeatedly with success. Some surgeons (17) prefer tutocain in a quarter per cent solution. I have tried it, too, but I could not find any advantages over novocain, or ascertain any protracted anæsthesia. Percaine is said by Christ Henschen to produce a longer (four to ten hours) anæsthesia, so that it is not only sufficient for the most difficult abdominal operations, but it also keeps the patient without pain for a long time after. It shows, however, the great dis-

advantage of being more poisonous and of causing necroses of the tissue, so that five out of 16 surgeons who had used it have given it up. I also abandoned percaine after having had two deaths within 24 hours in relatively simple operations. It is to be hoped that the pantocain pharmacologically tested by H. Schmidt and used by him in 1,157 cases, 167 of which were spinal anæsthesia inductions, will stand the test.

(To be continued.)

PERCAINE SUB-ARACHNOID BLOCK

By H. P. Fairlie, M.D.

Anæsthetist, Western Infirmary, Glasgow, etc.

PERCAINE is the mydrochloride of α-butyloxycinchoninic acid ethylenediamide. It is a quinoline derivative, belonging to a group of substances quite distinct from the cocaine group, and not previously known to have anæsthetic properties. Those properties were discovered some four years ago. It is a colourless, odourless, tasteless, crystalline substance, readily soluble in water and alcohol. Its anæsthetic potency is very high; it is ten times more powerful than cocaine and twenty times more powerful than novocain, while, in relation to stovaine, it ranks somewhere between these figures. Advantage of this powerful anæsthetic action of percaine has been taken to use it in very dilute solutions, thus reducing its toxicity. In a paper on this subject, the toxicity of percaine (*British Medical Journal*, Nov. 28th, 1931), Israels and Macdonald seemed to me to overlook the fact that extremely small doses of the drug are required for anæsthetic purposes.

Percaine has been used in many fields as a local anæsthetic, but I am confining my remarks to its employment as a spinal anæsthetic. It is to Howard Jones we owe its introduction in this sphere, and to his papers I refer you for a very able piece of pioneer work (*British Journal of Anæsthesia*, Vol. VII, and *British Medical Journal*, Sept. 12th, 1931). In my use of percaine I have followed Howard Jones's method, the feature of which is the introduction into the subarachnoid space of comparativey large quantities of a very dilute solution of the drug, very much larger than we have been accustomed to in spinal anæsthesia with stovaine and novocain. But I do wish to emphasise that percaine may be used in stronger solution—say one in two hundred—in exactly the same way as we have been accustomed to use these other drugs, i.e. injecting one or two cubic centimetres at the chosen site. Professor Queralla, of Turin, writes

enthusiastically of it as employed by this method, using a maximum quantity of 2.3 cubic centimetres of a one in two hundred solution made up with .9 per cent sodium chloride. He has had a very wide experience of spinal anæsthesia, commencing in 1912 and comprising some thousands of cases. As a result of his trial of percaine in four hundred cases he says:—"The use of percaine in spinal anæsthesia, especially for abdominal cases, yields results which are certainly superior to those which may be obtained with other anæsthetics (novocain, tropacocaine, stovaine, etc.). This is particularly the case with regard to the uniformity and the duration of the anæsthesia. It is also true of the perfect muscular relaxation which is obtained."

Personally, I have only recently had the opportunity of using percaine in this strength while, for nearly two years, I have been using it according to Howard Jones's method. The results with the latter have, on the whole, proved so satisfactory that I have not been strongly tempted to change. My remarks will be restricted to a description of percaine used in this way.

It is put up by the Clayton Aniline Company in 20 cubic centimetre ampoules containing percaine 1 in 1,500 with sodium chloride .5 per cent. The specific gravity of this solution is 1,003, i.e. lower than the specific gravity of the cerebro-spinal fluid, which varies between 1.005 and 1,008. The percaine solution is, then, lighter than the cerebro-spinal fluid, or hypobaric. In considering the question of injecting large quantities of anæsthetic solution into the subarachnoid space, it is worth remembering that the total amount of cerebro-spinal fluid in this space, from the level of the sixth cervical vertebra down to the extreme end of the cauda equina, is about 30 cubic centimetres. The quantity of anæsthetic solution to be injected depends on the level to which anæsthesia is desired. The essential difference between dilute percaine solution and other spinal anæsthetics lies in the fact that it is possible to inject at the third lumbar space enough of the drug to reach, in full strength, the nerve roots well up in the dorsal region. With other drugs this object is achieved in one of three ways: (1) by injection at the upper level of the nerve roots to be paralysed—a method which has obvious

dangers and has been discarded—(2) by injection in the lumbar region and reliance on gravity to carry the drug to the level required; or (3) by barbotage, i.e. mixing the drug in the syringe with cerebro-spinal fluid withdrawn and injection of this mixture.

For upper abdominal operations it is desirable to paralyse the nerve roots supplying the abdominal wall, and also the splanchnic communications involved in the viscera to be manipulated. This means that our drug should reach up to the fourth dorsal in order that the splanchnic supply to stomach and gall bladder may be included. Looming always in the background is the fear that the drug may travel too far upwards and interfere with the phrenic roots at the level of the fourth cervical. I now use 14 cubic centimetres as the maximum dose, which is less than the dose recommended by Howard Jones, but, I think, quite sufficient to obtain satisfactory anæsthesia for any abdominal operation.

For lower abdominal operations, from about the umbilical level downwards, 10 cubic centimetres is an adequate dose; and for a simple caudal block seven cubic centimetres suffices.

As well as reducing the dosage I have also been experimenting, latterly, with weaker solutions of percaine, and have found that a strength of 1 in 2,000, or even 1 in 2,500, produces a very satisfactory anæsthesia. I have not, however, had any of these cases tested as to the duration of anæsthesia beyond an hour and a half, and it is probable, as Jones suggested, that for a longer operation a stronger solution is necessary.

Since writing this I have had an opportunity of further testing the duration of anæsthesia. In an operation for fœcal fistula, involving resection of the caecum and 12 inches of ileum with lateral anastomosis, the incision extending from two inches above the umbilicus downwards, I gave 11 cubic centimetres of a 1 in 2,000 solution. The anæsthesia and relaxation were excellent until the end of two hours, when, at the commencement of the stitching of the abdominal wall, the patient complained of acute discomfort and the abdominal muscles tightened. Ether was administered to produce relaxation until the peritoneum was closed. The remainder of the stitching did not present any difficulty, nor were any further complaints made.

The series of cases on which I have based my remarks numbers 150, and of these 55 were high laparotomies, 86 low laparotomies, and nine caudal blocks. For convenience in considering these cases I should like to divide them into two categories: (1) the high laparotomies, and (2) the others. There is a number of features common to all which it would, perhaps, be better to deal with first.

In the first place my experience leads me to the conclusion that there are few patients able to go through the ordeal of a major operation in a state of normal consciousness. The nervous tension which results from the attempt is, I am sure, deleterious both to the patient and to the quality of the surgery performed. My routine is to give one-sixth of a grain of morphine by hypodermic, and from one and a half to four and a half grains of nembutal by mouth half an hour before the patient comes to the theatre. This combination, in the best circumstances, produces a fairly deep sleep and complete amnesia, commencing about twenty minutes after injection and lasting for about an hour and a half. It has the advantage of causing less fall of blood-pressure than most of the other narcotics—a point of considerable importance in connection with spinal anæsthesia. In addition to this I think that, in many cases, it is advantageous to give gas and oxygen. Unfortunately, the movement of the patient from bed to theatre, followed by the spinal injection, may spoil the action of the preliminary narcotic. I find that if gas and oxygen is administered for ten minutes the patient is again put to sleep, and often remains sleeping for the remainder of the operation.

As regards the actual technique of the injection the only point which needs emphasis is the use made of gravity. (1) The patient should be in the lateral position, and not sitting up. (2) A slight degree of Trendelenburg should be adopted immediately after injection. These two measures are adopted as safeguards against the light percaine solution rising upwards and reaching an undesirably high level in the spine, i.e. they restrict the diffusion of the percaine solution. (3) A point on which Howard Jones insists strongly is that the patient should be placed on his face immediately after the injection and kept there for from five to ten minutes. This is done to ensure that the posterior sensory nerve roots get adequately

soaked with the solution. If the patient be placed on his back immediately after injection the argument is that the light anæsthetic solution tends to come more in contact with the anterior motor roots, and that the posterior roots partially escape. In such cases a very complete relaxation ensues along with an imperfect anæsthesia. Jones attributes many of the failures in spinal anæsthesia, not only in the case of percaine, to the want of appreciation of the part played by gravity.

I almost invariably inject at the third lumbar space. After insertion of the needle only a few drops of cerebro-spinal fluid are allowed to escape, just suffiient to be sure that the point of the needle is through the membranes; then the required amount of solution is injected. This prevention of escape of cerebro-spinal fluid plays, I am sure, a very large part in the absence of headache folowing anæsthesia. Immediately after the injection a dose of ephedrine, or other similar drug, is administered. I give hypodermically one or one and a half grains of ephedrine. It counters, to some extent, the depressing action of the spinal injection, and diminishes the fall of blood-pressure.

1. *High Laparotomies.*

Of the 55 cases in this group 17 were stomach operations, 14 gall bladder, 2 spleen, 4 intestinal obstructions, 4 ventral herniæ, 4 resections, and 9 explorations. The ages ranged from one month to 85 years. My earlier experiences corroborated the very glowing reports I had read of percaine, and I began to think that all the difficulties of anæsthesia in upper abdominal surgery had been overcome. As time went on, however, my views underwent modification. In the first place, dosage is a difficulty. Jones advocated 16 to 18 cubic centimetres of a 1 in 1,500 solution, with the object of ensuring a splanchnic block by reaching the level of the fourth dorsal. I think that he has since somewhat modified his dosage. I have, latterly, been working with 14 cubic centimetres as a maximum dose; less for patients of shorter stature. With the larger doses I found that the fall of blood-pressure was so considerable in some cases as to make me anxious, and, although I have not seen a death, I felt that too great risks were being run. In a certain number of cases, irrespective of dosage,

i.e. even in my earlier experiences when I was using 16 or 18 cubic centimetres, I found that traction on the stomach or gall bladder caused marked discomfort and pulse failure, and that it was often desirable to administer gas and oxygen to tide over this stage. Now with smaller quantities of percaine, and in some cases with strengths of 1 in 2,000 or 1 in 2,500, I find the danger is greatly reduced. Gas and oxygen is an excellent aid over a difficult time. It does not depress the vasomotor or the respiratory mechanisms. It is necessary to remember, however, that the oxygen content of the mixture must be kept higher than in an ordinary gas-oxygen anæsthesia, as the deprivation of oxygen more readily causes respiratory failure in this form of spinal anæsthesia.

The principal advantages obtained by percaine in those operations were: (1) the very complete muscular relaxation which results, and the correspondingly high degree of operative facility; and (2) the relatively comfortable post-operative period with less sickness than after a general anæsthetic. As regards respiratory sequelæ I have been somewhat disappointed with the results. Out of the 55 cases there were nine in which chest complications of various kinds ensued. Three of those were of pneumonia; two patients died, one on the third day and one on the ninth day. The other six included collapses of the lung and bronchitis; all recovered. A probable explanation of this incidence of respiratory complications is the intercostal paralysis, with resulting defective lung ventilation, which accompanies high anæsthesia.

I think it is reasonable to say that high percaine anæsthesia offers marked advantages in : (1) very muscular patients who have to undergo stomach or gall-bladder operations; (2) stout patients with large ventral herniæ, where the relaxation is an asset; and (3) patients with acute intestinal obstruction accompanied by copious, perhaps fœcal vomiting, where the danger from inhalation of stomach contents is so imminent in general anæsthesia. There is one other group of cases in which, I think, it might have great advantages—high laparotomies in infants, e.g. in congenital pyloric stenosis and intussusceptions. My experience in this field is restricted to two cases, children of five weeks and thirteen

months, and, although satisfatory, too limited to form any conclusions.

2. *Low Anæsthesiæ.*

These include prostatectomies, hysterectomies, ovarian cysts, and appendices, 86 in all, while nine others were caudal blocks for excisions of rectum and colpo-perineorrhaphies.

With the smaller doses of percaine demanded in this class the risks are correspondingly reduced. A dose of 10 cubic centimetres is sufficient for a prostate or low laparotomy, and six or seven cubic centimetres for a caudal block. The fall of blood-pressure is less marked, while chest complications are fewer. Out of the 95 patients there were six who developed serious chest symptoms, one of whom died—an old man of 84—after prostatectomy. Curiously enough the sickness incidence is higher in this series than in the first group, especially after hysterectomy, but even there it is lower than after general anæsthesia.

With regard to remarks applicable to my general experience with percaine over all the cases, in the first place I think its efficiency ranks high. In one of my cases there was complete failure to get anæsthesia; while in six other cases there was imperfect anæsthesia. In respect of the sequelæ which are especially associated with spinal anæsthesia I have found it almost immune. Severe or lasting headache was not present in any case, nor were there any of the special nerve affections, such as squint or deafness. None of the patients developed spinal tabes. One rather troublesome sequel which did occur in two cases was hiccough, once after gastro-enterostomy and once after prostatectomy, in the latter case lasting ten days.

With the proviso, which, in my experience applies to spinal anæsthesia in general, that percaine should be used only when the heart and circulation are functioning reasonably well, I think it is fair to claim for it a very satisfactory anæsthesia and relaxation, of longer duration than is obtainable with other drugs, and a greater freedom from unpleasant sequelæ.

TETANY DURING ETHER ANÆSTHESIA

By W. N. Kemp, B.A., M.D.

From Anæsthetic Staff of the Vancouver General Hospital, Vancouver, B.C.

FOR a number of years, at the Vancouver General Hospital, we have noted the occasional occurrence of twitching of the facial muscles in patients under ether anæsthesia. Usually the twitching becomes very marked and may extend to the muscles of the arms and even the abdominal muscles. The muscular movements may be described as vigorous clonic contractions, which we will call tetany in the discussion. The cause has not been satisfactorily explained in the literature. The common and most plausible explanation is that the tetany is due to impurities in the ether. This is probably not the real explanation, for the following reasons: (1) If impurities in the ether were the cause of tetany in one patient, in a large general hospital, the cases of its occurrence would not be limited to one in a definite period of time; (2) chemical examination has not shown any undue impurities in cases reported elsewhere.[1] (3) the writer has noticed that if an individual develops tetany during operation under ether anæsthesia, the same individual will again develop tetany if he is reoperated on at a later date. These observations would tend to show that the onset of tetany under ether anæsthesia is due to factors inherent in the patient himself. The following case history is illustrative of this point:

Mr. A. V., an adult male, aged 48, admitted to hospital February 14th, 1932, with a diagnosis of perforated gastric ulcer. A laparotomy was immediately performed under ether anæsthesia (intratracheal ether with oxygen). Anæsthesia was induced at 10.30 a.m. The induction of anæsthesia was quiet and even, as was the course, until 11.10 a.m., when the operator was closing the abdomen after simple closure of the perforation, when twitching of the facial muscles commenced.

On the assumption that this twitching it akin to the tetany of over ventilation, the patient was allowed to rebreath oxygen and his own carbon dioxide through the medium of the gas machine and rebreathing bag. This procedure checked the twitching of the facial muscles. The examination of a catheter specimen of urine, obtained 15 minutes later, at the end of the operation, showed an alkaline urine which was otherwise normal. The patient made a good recovery, and on April 5th a partial gastrectomy was scheduled. Anæsthesia was again normal until about 15 minutes after the introduction of the intratracheal ether-oxygen catheter (30 minutes after the commencement of anæsthesia) when there was a recurrence of the facial twitching noted in the former anæsthesia. Carbon dioxide rebreathing was induced by covering the face with an ether mask and damp cloths in the manner of a semi-open ether anæsthesia. In a few minutes the twitching ceased. Respiration continued as it had always been during this and the previous operation at the normal rate for intratracheal ether-oxygen anæsthesia. If the face mask were removed for five minutes the twitching would recommence. Unfortunately, owing to technical difficulties, an immediate post-operative catheter specimen of urine was not obtained. A specimen obtained 10 hours post-operative was acid in reaction and normal in every respect.

With this and a few other cases as a basis, the writer would suggest that the facial twitching (tetany) sometimes seen under anæsthesia is due to a temporary alkalosis, which is caused by the excessive loss of CO_2 (excessive for the individual) and the consequent upset in his blood pH. In support of this view the following explanatory paragraphs are appended.

"Nothing is more constant in the animal economy than the H-ion (pH) concentration of the fluids which bathe the tissues. The regulation is fundamentally of a physicochemical nature, depending on the interaction of alkalies with acids, of which carbonic and phosphoric are the most important. Under certain circumstances proteins may also act either as acids or as alkalies. The neutralizing properties of proteins are, however, of little consequence in the neutrality regulation in the human body.[2] When different amounts of acids

or alkalies are added to water, the range of variation in H-ion is very extensive, whereas in blood the range is very limited indeed, not extending beyond *p*H 7 and *p*H 7.52 (i.e. H-ion concentration never goes above that of a 0.000,000,1 normal solution or below that of 0.000,000,03 normal solution). In other words blood can withstand considerable additions of acid or alkali without much change." Macleod.[3]

The chemical reactions upon which this remarkable constancy of reaction (H-ion concentration) depends have been explained by L. J. Henderson.[4] This constancy of *p*H is due to the presence in the blood of "buffer substances" of which $NaHCO_3$ is one of the most important. The ratio between the amount of carbonic acid (H_2CO_3 or $CO_2 + H_2O$) and the basic mono-sodium carbonate ($NaHCO_3$) is the most important factor in the control of the H-ion concentration, because it can be instantly changed through the elimination or accumulation of CO_2 by way of respiration. In other words, by alterations in the $H_2CO_3 \div NaHCO_3$ ratio, rapid accommodations in *p*H can be made through the lungs. The ratio may be increased either by adding some free carbonic acid to the blood (as by causing the respiration of some carbonic acid gas—CO_2) or by the addition of some other acid (e.g. oxybutyric, as in diabetes) which will decompose some of the $NaHCO_3$ and thus produce H_2CO_3. The increase which these changes would cause in *p*H of the blood is prevented by the remarkable sensitivity of the respiratory centre to such changes. When the rise occurs in the centre it is stimulated, causing increased pulmonary ventilation, so that the carbonic acid is immediately eliminated through the lungs. When the increase in H_2CO_3 is due to the presence of some fixed acid the elimination of CO_2 does not stop when the old level of carbonic acid concentration is reached, but proceeds until the original ratio $H_2CO_3 \div NaHCO_3$ is again attained in the blood, and *p*H is restored exactly to its original value. If it stopped at the old CO_2 concentration the ratio would be too high, because there is now less $NaHCO_3$.

When plasma is obtained by removing blood from the vein of the arm and centrifuging it immediately out of contact with air (so that CO_2 may not be lost from it) it contains approximately 60 volumes per cent of CO_2. Since we know

that the partial pressure of CO_2 in blood is equal to 42 mm. Hg.[5] (ascertained from determinations of the alveolar CO_2) we can calculate how much of the 60 volumes per cent must be in simple solution, by application of the law of solution of gas in a liquid. One cubic centimetre of plasma at body temperature and at 760 mm. Hg. (atmospheric pressure) dissolves 0.541 cubic centimetres CO_2 (coefficient of solubility in blood at 38° C.), so that at 42 mm. it will dissolve $42 \times 100 \times 0.54 \div 760 = 3$ volumes per cent. That is, of the 60 volumes per cent of CO_2 present in normal blood, three volumes per cent is in simple solution in the plasma. Transscribing the figures to our equation we get $H_2CO_3 \div NaHCO_3$ equals $3 \div 60$, or $1 \div 20$.

The Respiratory Quotient (R.Q.) is defined as the ratio existing between the volume of CO_2 expired and the volume of O_2 retained by the tissues. That is

$$\text{R.Q.} = \text{Vol. } CO_2 \text{ expired} \div \text{Vol. } O_2 \text{ retained}$$

The Respiratory Quotient (R.Q.) varies in health with the diet, as it is an indicator of the kind of combustion that is occurring in the organism. The R.Q. for carbohydrate is exactly unity; for fat it is 0.71; for protein 0.82. Hence the R.Q. of a healthy individual, on a mixed diet, is between 1 and 0.71.

If a man breathes forcibly and quickly for about two minutes, he will experience no desire to breathe for a further period of about the same duration—he becomes apnœic. When the desire to breathe returns, the breathing is at first shallow, and frequently periodic in type, but gradually becomes more marked, until at last normal respiration is re-established. Every anæsthetist with any experience in the use of ether has noticed this phenomenon upon the operating table. The cause of the absence of breathing (apnœa) is a lowering of the CO_2 tension in the blood and, consequently, in the respiratory centre. The CO_2 tension is lowered because this gas has been "washed out" or "blown off" from the blood into the over ventilated alveoli. Although this over ventilation also raises the pressurs (tension) of O_2 in the alveoli, little, if any, more of this gas can be absorbed into the blood because, even at the normal alveolar tension, the blood takes

up at least 95 per cent of its possible load, in accordance with the dissociation curve. These differences in the rate of CO_2-loss and O_2-gain cause the respiratory quotient to become very high during the forced breathing; it may, indeed, rise to nearly two. If, during the apnœic period, the man, by an effort expires some alveolar air, and, if this be analysed, it will be found that very little CO_2 is being expelled from the blood, though the O_2 is being absorbed as usual. Consequently the R.Q. becomes very low (0.2 has been observed). As a result of this disproportionate loss of CO_2 the numerator of the equation $H_2CO_3 \div NaHCO_3$ becomes smaller, thus upsetting pH towards the alkaline side. A temporary alkalosis is thus established. This alkalosis is revealed by a great increase in the excretion of alkali in the urine and a corresponding decrease in ammonia. The $NaHCO_3$ (representing the denominator of our ratio) must be decreased in order to keep the essential constant ratio of $H_2CO_3 \div NaHCO_3 = 1 \div 20$. Basic substances, associated with $NaHCO_3$ can be eliminated in the urine; hence the alkalinity of the urine is a measure of the degree of alkalosis in the body. The urine may become alkaline if the forced breathing is kept up for as short a time as 20 minutes. In the physiology laboratory a most interesting train of symptoms, known as tetany, supervene after 20 minutes of forced breathing.[6] The first of these is twitching of the facial muscles when the seventh nerve is excited, by tapping it as it crosses the zygoma. Later this twitching (tetany) may become spontaneous, and meanwhile other symptoms, such as visual impressions and sensations of heat and cold, may supervene, accompanied by a curious fixedness of ideas which often makes it difficult to persuade the person to discontinue the forced breathing. Collip[7] and Grant and Goldman[8] consider the condition of alkalosis induced by the forced breathing to be responsible for the tetany.

The conditions of forced breathing, as outlined in the above laboratory experiment, are duplicated in the administration of an intratracheal ether anæsthesia, especially when oxygen is used as a vehicle to carry the ether vapour into the trachea. One is therefore justified in the conclusion that the clonic

muscular movements sometimes seen in non-epileptic individuals, under open or intratracheal ether anæsthesia, is due to a general alkalosis induced by the rapid elimination of CO_2 and the consequent severe upset in the normal respiratory quotient.

REFERENCES.

1. Sykes, S. *Brit. Med. Journ.*, 1930, i, 1128.
2. Bayliss, W. M. "Neutralization," etc.
3. Macleod, J. J. R. "Physiology and Biochemistry in Modern Medicine," 1930, p. 32.
4. Henderson, L. J. "The Excretion of Acid in Health and Disease," Harvey Lectures, L. B. Lippincott, Co., 1915, 10, 132.
5. Macleod, J. J. R. "Physiology and Biochemistry in Modern Medicine," 1930, p. 34.
6. Idem., ibid., 586.
7. Collip, J. B., and P. L. Backus. *Amer. Journ. of Physiol.*, 1920, li, 568.
8. Grant, S. B., and A. Goldman. Ibid, 1920, lii, 209.

MODERN ANÆSTHETICS, THE TEACHING HOSPITAL AND THE UNDERGRADUATE

By J. Ross Mackenzie, M.D., Aberdeen.

THE dilemma of a recent medical graduate, who, as revealed in his letter to the Editor of the *British Journal of Anæsthesia*, of April, 1932, has seen much of expert anæsthesia but has never administered an anæsthetic of any kind, gives cause for serious reflection by a large body of practising anæsthetists. The status of anæsthetics in the medical curriculum, the attitude of surgery generally to the subject of anæsthetics, the standard of training in the science and practice of anæsthesia required for appointment as an honorary anæsthetist, and the work in anæsthetics required by the medical student for graduation have urgent need to be revised and put on an equitable foundation.

The tendency in the modern, well-equipped hospital is for a still larger proportion of the surgical work to be done under expert methods of gas and oxygen anæsthesia. That the benefits which accrue to the patients are of importance is supported by surgeons and physicians and is confirmed by the surgical ward sisters, who are unanimously of opinion that gas and oxygen anæsthesia has done much, not only to ensure a rapid and uninterrupted convalescence to the average surgical patient, but also to reduce the post-operative work of the surgical nursing staff.

The practice of anæsthetics in hospital requires to be modified accordingy as the hospital is or is not a fundamental part of a teaching, medical school. The demands of the patient and of the surgeon for the anæsthetic agent, and the method of its administration, at once safest, most efficient and least likely to disturb convalescence or to initiate post-operative complications are opposed to the claims of the undergraduate.

In the interests of anæsthetic service to the public it is essential that the medical student should be taught methods

of efficient anæsthesia which require simple apparatus and portable agents. It would appear that a considerable percentage of the anæsthetic mortality of to-day is unnecessary and is directly attributable to the inexperience of the administrators. This is a problem which teaching schools must consider and rectify, since new graduates are certified, without examination and possibly without any practical experience, as fit and proper persons to administer any kind of anæsthetic agent. The practitioner, therefore, administers and will continue to administer anæsthetics in various branches of surgery, and there are surgical patients who consent to anæsthesia on condition that the anæsthetic is administered by their own doctor. Such patients dread the anæsthetic more than the operation, and they are firmly convinced that their safety is assured if the administration is in the hands of their family physician. This is a psychological phenomenon which flatters the practitioner, but it must be admitted by anæsthetists that, other things being equal, mental equanimity and confidence in the administrator are important elements in the safe administration of anæsthetics.

My own practice has been the *via media*. Neither the interests of the patient, the surgeon, the undergraduate nor the science of anæsthetics need be wholly sacrificed. If the condition of a patient or the nature of an operation demand expert methods of anæsthesia, the undergraduate must not be considered, but a balance is struck in his favour by the administration of open or closed ether or ethyl chloride and ether or chloroform to surgical cases such as an interval appendicectomy, herniotomy, enucleation of tonsils or gastro-enterostomy. A high degree of concentration must be demanded from the undergraduate when engaged in practical anæsthetics under supervision. The attitude of indifference to the subject, the low estimate of its importance and difficulties, along with an impression that efficient surgical anæsthesia is accomplished and maintained in each patient by a definite formula, have to be corrected in the average undergraduate. The administration of chloroform is more calculated to promote meticulous care and judgment in the undergraduate, and to train him in the proper use of powerful narcotics, than any other anæsthetic agent.

A more difficult aspect of this problem is the crushing effect on the teaching of anæsthetics of the ever-increasing medical curriculum and the overwhelming importance of medicine, surgery and allied subjects for examination purposes. The close relationship between surgery and anæsthetics would appear to justify an occasional question on the subject of anæsthesia either in the surgery paper or in the clinical examination. The adoption of this reasonable suggestion by examining surgeons and physicians generally would immediately result in a vast improvement in the standard of knowledge and experience of practical anæsthetics by the undergraduate. It would increase the margin of safety of the administration of anæsthetics to the public and assist in procuring for anæsthetics and the anæsthetist their proper position in surgery.

In order to merit, consolidate and maintain such a position in the surgical unit, three important advances must be accomplished in anæsthetics without delay.

1. The anæsthetist must render indispensable service to surgery by bringing to bear on each surgical patient a high degree of judgment, knowledge and skill in the pre-operative preparation, in the choice of the anæsthetic, in the technique of expert anæsthesia and in the prophylaxis and treatment of post-anæsthetic complications.

2. The future of anæsthetics demands that a definite standard of knowledge of the theory and practice of anæsthetics and of allied essential subjects such as physiology, biochemistry, pharmacology, anatomy and laboratory work be required of anæsthetists for hospital and teaching posts. The time is ripe, and should not be delayed, for the serious consideration of the institution of a Diploma in Anæsthetics.

3. A financial return commensurate with such an anæsthetic service is essential to attract graduates likely to advance the science and art of anæsthesia.

A NEW ASSOCIATION OF ANÆSTHETISTS.

BEFORE the next number of this journal appears there will probably be in existence an organisation which will have great interest and be of much service to our readers in general. At a preliminary meeting held at the rooms of the Medical Society, in Chandos Street, on April 28th under the chairmanship of Dr. Featherstone of Birmingham, a resolution was passed to form an Association of Anæsthetists of Great Britain and Ireland, and a provisional committee of ten was elected, with power to add to its numbers up to fifteen. This committee consists of :

Chairman : Dr. Featherstone.
Secretary : Dr. Howard Jones.
Treasurer : Dr. Z. Mennell.

with Sir Francis Shipway, Mr. H. E. G. Boyle, Mr. Ashley Daly, Mr. Ramsey Phillips, Mr. T. H. Chaldecott, Dr. Faulkner Hill and the Editor of the *British Journal of Anæsthesia*. Subsequently the following five gentlemen were co-opted to join the provisional council. C. H. Hadfield, R. E. Apperley, H. P. Fairlie, H. Torrance Thompson, J. Ross Mackenzie.

The exact objects of the prospective association, with suggested rules and regulations, are to be circulated to a number of anæsthetists who will be asked to attend the first general meeting. At present we can merely state that the objects of the association are to be primarily political and economic, the scientific aspects of our specialty being already sufficiently catered for by the Anæsthetics Section of the Royal Society of Medicine and other bodies. It is believed that numerous questions of paramount interest to anæsthetists arise from time to time and cannot be discussed by the bodies just mentioned because they limit themselves to the purely scientific or technical side of anæsthesia. When individual anæsthetists or managing bodies for hospitals want advice on difficulties arising in connection with, for example, the teaching arrangements, or the remuneration of anæsthetists, there is no one body to whom they can look for authoritative guidance. The want of such a body has been felt, too, in connection with the proceedings of Coroners' Courts. It is hoped that the new association, whose membership is to be widely representative, will meet these requirements and do for anæsthetists something of what the Association of Pathologists and the Association of Surgeons and the Association of Physicians have achieved for the members of those successful organisations. We hope to record in a future number of the journal the formation and the first activities of the new association.

ANÆSTHETIC SECTION.

(Royal Society of Medicine).

At the Annual General Meeting, on Friday, May 6th, the following officers were elected for the Session 1932–1933.

President :—C. F. Hadfield, M.D.

Vice-Presidents :—R. E. Apperly, M.R.C.S.; H. W. Featherstone, M.D.; G. Ramsey Phillips, M.R.C.S.

Hon. Secretaries :—R. J. Clausen, *M.C.*, M.B.; W. Howard Jones, M.B.

Other Members of Council :—C. Langton Hewer, M.B., Cecil Hughes, O.B.E., M.B., J. F. Ryan, M.B., I. W. Magill, M.B., R. J. Minnitt, M.D., C. W. Morris, M.B.

Representative on Library Committee :—

Sir Francis E. Shipway, *K.C.V.O.*, M.D.

Representative on Editorial Committee :—

J. Blomfield, *O.B.E.*, M.D.

ABSTRACTS

An experimental study of the detoxicating properties of sodium thiosulphate in avertin intoxication. A. BOLLIGER. *Med. Journ. of Australia.* Jan. 23rd, 1932, p. 125.

Intravenous or sub-cutaneous injections of sodium thiosulphate increase the tolerance for avertin by as much as 50 per cent of the minimum lethal dose, but there is no stoichiometric relation between the amount of antidote and the increased tolerance for the toxin. As a restorative in case of emergency sodium thiosulphate, given intravenously, may be of use if used early enough, but rectal lavage was found more effective. Sodium thiosulphate in about 30 per cent solution given rectally at the same time as avertin protects against more than twice the lethal dose of the drug. Even with a 10 per cent solution detoxicating effects were noticeable. In these experiments, when anæsthesia resulted, the period of induction, as well as the period of recovery, was prolonged. This is in all probability due to delayed absorption of the avertin in the presence of the hypertonic solution of sodium thiosulphate. The fact that with a high concentration of the thiosulphate it is impossible to induce anæsthesia may be explained by the assumption that the absorption of avertin is so much delaped and diminished that a sufficient concentration of the anæsthetic cannot be reached. Only analgesia may be obtained. Probably when two crystalloids are given rectally each may influence the absorption of the other in proportion to their respective amounts present. Practically this delaying action of thiosulphate may be of use in obstetrics. In overdose, high rectal irrigation with warm hypertonic thiosulphate solution acts as a restorative.

Observations on experimental spinal anæsthesia. W. K. FERGUSON and J. F. NORTH. *Surg. Gyn. and Obstet.,* March, 1932, p. 21.

The authors state that very little experimental work has been done on the effect of injections into the spinal canal, most of our knowledge resting on clinical observation. They were particularly concerned to test the current theory that the drop in blood-pressure, so constant during spinal anæsthesia, is due to accumulation of blood in the splanchnic area, due to paralysis of the vasomotor nerve supply. They refer to the fact that a much more profound depression has been found with spinal than with splanchnic anæsthesia, an argument against the accepted theory, and they state that the assertion that "division of the splanchnic nerves produces a profound fall in general blood-pressure" rests on questionable grounds. The

suggestion made by Gray, Parsons and Featherstone that the low blood-pressure may be due to accumulation of blood in the capilllaries of the relaxed muscles is difficult of acceptance in view of the fact that extreme relaxation under general anæsthetics should show an equal fall in blood-pressure, but does not. The experiments were carried out on dogs which were put under ether for the necessary operative exposures. The subarachnoid injections were made through a laminectomy opening. Novocain was the drug employed.

The summarized conclusions arrived at from the whole number of experiments were :—

1. Accompanying a typical anæsthesia there is a fall in blood-pressure averaging 56 per cent, a slowing of the pulse-rate and a diminution in the respiratory movements of the thorax.

2. The fall in blood-pressure cannot be explained on the basis of a splanchnic paralysis. Division of the splanchnics produces only a slight alteration in the general blood-pressure, and the typical fall of spinal anæsthesia can be produced in animals with both splanchnics completely severed. The entire vasomotor system participates in a vasodilation, and the degree of blood-pressure depression is in direct ratio to the number of rami anæsthetised.

3. The fall in blood-pressure may be greatly ameliorated by the vaso-constrictor effect of preliminary medication with ephedrin.

4. The retardation of pulse-rate is probably due to the interruption of impulses in the cardiac accelerator nerves.

5. Paralysis of motor fibres to the intercostal nerves seems to account for the respiratory depression.

6. Complete suppression of respiration occurs when novocain reaches the cervical portion of the cord and the medulla in sufficient concentration to paralyze the phrenic or the respiratory centre.

Anæsthesia in the tropics. A. BARNSLEY. *Malayan Medical Journal.* March, 1932.

The author was, for three years, anæsthetist to the General Hospital, Singapore. The atmosphere of Malaya is almost completely saturated with moisture, and this interferes with open inhalation methods. An ounce of ether, which would take twenty minutes to evaporate in England does so in eleven minutes at Singapore. Ether is thus more effective than in Bombay, where evaporation of the same amount takes only five minutes. The moisture, however, makes the administration of open ether inconvenient and, in addition, the dread of the natives of the ritual of any inhalation caused Dr. Barnsley to welcome the advent of avertin. This he has found highly satisfactory and he states that the agent will increase the efficacy of surgery in tropical countries by a hundredfold.

L'Anesthésie a l'avertin et au protoxyde d'azote-oxygène combinés. Professor Demerest. *Revue médicale*, Feb. 17th, 1932.

Professor Demerest, on the basis of an experience of 200 cases, gives warm approval to the combination of avertin with gas oxygen. He finds that the inhalation serves admirably to counteract respiratory depression or fall of blood-pressure which may be brought about by the injection of avertin. He believes in slow injection and a dosage smaller than that commonly employed in this country. In determining the dose he is only slightly influenced by body weight, paying more attention to the state of health and muscular and nervous constitution of the patient. He never aims at anæsthesia from the avertin alone. Roughly speaking the dosage is the patient's weight multiplied by 0.09 in the case of men and 0.08 in the case of women. He never exceeds 8 grs. In order to get the proper percentage solution he multiplies the number of grains of avertin by 40 to get the requisite amount of water, e.g., if the patient were to have 4 grs. of avertin it would be dissolved in 160 c.c. water, or 4 c.c. liquid avertin in 160 c.c.

Blood-pressure in avertin anæsthesia. Messrs. Taylor and Hunt. *N.E. Journ. Med., March* 24th, 1932, p. 613.

The authors think that insufficient attention has been given to the fall in blood-pressure which may occur after avertin injection. In their first 50 cases, with an average pre-operative blood-pressure of 125 m.m. cystolic, 65 m.m. diastolic, the average post-operative pressure was 85 m.m. cystolic, 45 m.m. diastolic. Usually the clinical state of the patient gave no indication of this marked drop. The pulse-pressure remained satisfactory and the ratio of pulse-pressure to diastolic pressure showed very little disturbance. The response to a single dose of ephedrine was prompt and there was no tendency to a second drop when its action had worn off. The authors contend that the fall of blood-pressure which they record is not due to surgical shock because the venous oxygen content is always within normal limits, whereas in surgical shock there is a reduction of two to four volumes per cent of venous oxygen.

Sodium amytal. Analysis of intravenous use in 132 operations. Hugh Cabot, H. G. Maddock, and H. Lamb. *Archives of Surgery.* May, 1932.

The authors' general conclusions are that sodium amytal, given intravenously, is a satisfactory addition to the anæsthetic armamentarium, that it is exceedingly agreeable from the patient's point of view, and that, as 75 per cent of the patients need additional anæsthetic small doses of amytal would be just as satisfactory as the nearly maximal dose employed. Careful observation of the times needed for the patient to regain consciousness sufficiently to take

fluids by the mouth showed that this occurred within 12 hours in 86.2 per cent of the cases and within four hours in 26.4 per cent. The longest time for recovery to this extent was 46 hours.

Inhalation anæsthesia by a gravitational method. H. H. MILLER. *Anæsth. and Analg.*, March and April, 1932.

The author has revised and elaborated the method, practised years ago by Flux in London, of pouring down nitrous oxide: "An open cone is prepared from newspaper and covered with a towel. An evaporating chamber is arranged by having a diaphragm of gauze, held by an adjustable metal ring, two inches from the distal end of the cone. In the chamber is placed a wad of gauze to serve as an evaporating surface. It the cone is eleven inches high, a rebreathing space, two inches deep, is left below the evaporating chamber." This space is warmed by the expirations. A quarter-inch metal tube, five inches long, bent to a hairpin curve, and connected by rubber tubing to a gas oxygen machine is hooked on to the distal end of the cone.

In the same journal, at page 63, H. Widenhorn draws attention to the use of coramin in connection with avertin anæsthesia, and this has appeared elsewhere also in recent literature. He states that it is established that coramin, of all circulatory stimulants, possesses the strongest action and the safest therapeutic applicability. It is indicated in all lesions of the central nervous system, especially of the medulla oblongata and the vital centres. In deep avertin narcosis, with reduced breathing and low blood-pressure, improvement takes place within a few minutes of intravenous coramin. Widenhorn, who has experience of 1,500 cases of avertin anæsthesia, recommends that in cases of danger 6 c.c. of a high percentage of aqueous solution of coramin be introduced by the skin. As a routine to secure hyperventilation and abridgment of post-operative sleep he recommends 2 to 3 c.c. and an intra-muscular injection of 5.c.c.

A new local anæsthetic. New England Journal of Medicine, April, 1932, p. 759.

At the American Chemical Society, on March 27th, V. H. Rider and E. W. Scott described a new local anæsthetic "diothane" as being stronger than cocaine and satisfactory for use in any operation under local anæsthetic. It is not habit-forming and is less toxic than novocain. Chemically the new agent is hydrochloride of piperiddion propanediol di-phenylurethane.

Avertin-ethylene anæsthesia. H. B. NEFF. *Canadian Med. Assoc. Journ.*, May, 1932, p. 576.

The author reports very favourably on 50 cases. He used morphia and atropine before the avertin and in most instances added 30 c.c.

of 20 per cent mag. sulph. solution to the avertin solution. Ethylene and oxygen were given at the rate, on an average, of 3.5 and 1.5 litres per minute. In long abdominal operations a small amount of ether was found necessary to facilitate closing the peritoneum.

Kirchbauer and Maisink (*Clin. Med. and Surg.*, May, 1932, p. 351) report 200 cases of avertin anæsthesia. They regard the method as the best of all for goitre operations.

CORRESPONDENCE

To the Editor,

British Medical Journal.

Sir—I read, with, interest, the letter from Dr. Bessie E. Cook in your number of January 30th, dealing with a passage in Dr. Blomfield's textbook on anæsthetics and deaths associated with anæsthesia.

At the present time there is a marked tendency among anæsthetists to devote themselves to stopcocks, valves and percentages—when I have seen any marked improvement amongst these administrators I will be perfectly willing to admit their superiority—so far I have not noticed any superiority, but rather an inferiority in that they devote the major portion of their attention to their apparatus and not to the patient. The simpler the method of administration the better the administrator and the better for the patient, provided the administrator knows his job.

In spite of all the outcry at the present time claiming unsurpassed excellence for nitrous oxide, ethylene, and other methods of general anæsthesia, there is nothing to equal the correct administration of the major anæsthetics, chloroform and ether. Provided the patient is *led* into a stage of surgical anæsthesia, and provided the patient has had correct premedication, chloroform and ether surpass any of the more modern or popular anæsthetic methods at present in vogue. There is no strain thrown on the heart under correct chloroform induction if the patient has had a preliminary injection of morphia and atropine—but under every circumstance the patient must be led and not uppercut into a stage of surgical anæsthesia. The chloroform mask should never be brought into close contact with the face, and after the patient has been dulled under the chloroform vapour, ether should always be added.

The correct administration of any general anæsthetic is not anæsthetic coma but anæsthetic sleep attained by the administration of chloroform and ether, plus premedication.

What is claimed by the advocates of nitrous oxide and ethylene can equally well be obtained by chloroform and ether, and with better muscular relaxation, provided the anæsthetist knows his work—also with the rousing of the patient, even after a two hours or more administration.

Without either post-operative vomiting or any risk of post-operative pneumonia—mechanical methods of anæsthetic administration have not lessened our anæsthetic deaths, nor have they led to a better administration of anæsthetics.

Yours faithfully,

(*Signed*) R. W. Hornabrook.

Glenferrie Road,
Malvern,
Melbourne.
April, 4th, 1932.

To the Editor,

British Journal of Anæsthesia.

Sir—I am writing this in bed—they have leg-roped me all right for the next month—not permitted to do anything. The fact is my old heart appears to have kicked against the pricks.

For some time past I have thought more than once that things were not all they should be, but put the pains down to possible indigestion or gastric trouble.

The attacks are original in character, last about five to ten minutes, but are a little bit of hell, at times, in their severity. They are generally accompanied by one very interesting symptom, severe pain at the *back* of the *right* hand and sometimes also at the *right* elbow; the acute pain passes off with the attack but leaves a feeling of tenderness in the back of the hand. This gradually disappears and I feel my old self once again.

What I regret is that I have not been able to get a cardiograph tracing whilst one of these attacks is on. Some months ago I went through a series of CO_2 inductions. I had three at the hands of three separate men, at intervals of some days between each induction.

The interesting part is this: that after each induction I felt that *a bit of a strain* had been thrown upon my heart. It was entirely different from anything that I had felt under any other form of induction. I cannot explain it better than to say that my heart felt uncomfortable. This was so marked after the third experience that I went up to see a heart specialist, and he told me that he could not make out any special feature or abnormality.

A little later pains came on now and again—sometimes at intervals of some days. The pains have been more frequent lately, with the result that I am ordered to lie low for a month.

I am certain that these CO_2 inductions may be a source of very grave danger to patients. In, say, 999 cases out of 1,000 no harm happens or appears to happen, at least at the time. Nothing, however, will convince me that any induction which gives rise to these marked respiratory excursions, such as are seen with CO_2 inductions (excursions which grow rapidly in extent and depth) is not a very possible danger to the patient, especially in anyone past middle age or in an individual with a heart in anyway below par.

As I have repeated so often, patients should be led and not uppercut into a stage of surgical anæsthesia. It does not really matter how bad a patient's heart may be, provided a patient is led into an anæsthetic sleep. There is really no more strain thrown upon the heart than in going to sleep, especially so in the case of a patient who has received correct but not over-premedication prior to induction.

Rush inductions are an indication of bad work. I will admit that rush inductions are sometimes the fault of an impatient surgeon, and impatient surgeons are not unknown in our profession, especially if they have a reputation as skilful operators.

Yours sincerely

R. W. Hornabrook.

P.S. The X-rays reveal dilatation of the Aorta. (*R.W.H.*)

1 Wimpole Street,
London, W.1.
8th June, 1932.

Sir.—In reply to the letter published in your last issue, signed "Inquirer," I should be grateful if you could find space for the following details.

The Fellowship of Medicine has recently revived the arrangements which formerly existed whereby instruction in anæsthetics was available for postgraduates.

We have now in progress two courses of practical instruction given by two anæsthetists attached to the Staff of various general and special hospitals. The number of postgraduates permitted to attend each course is necessarily limited, so that full opportunity may be given to each postgraduate to receive real and practical help. I think these arrangements would meet the difficulties raised by your correspondent. All details can be obtained from the Fellowship of Medicine, and are also published from time to time in our official Journal.

Yours faithfully,
(*Signed*)*Maurice Davidson*,
M.D., F.R.C.P.
Hon. Sec. The Fellowship of Medicine and
Postgraduate Medical Association.